Recent developments from lattice QCD

Thomas DeGrand

Department of Physics, University of Colorado, Boulder, CO 80309 USA

This year lattice QCD has become very public. A new generation of simulations (including light dynamical quarks) have produced results which are in close agreement with many "easy" experimental quantities, and precise predictions for quantities which are tests of the Standard Model. Is QCD over? Reality is somewhat more nuanced. I will try to put the recent results into context: as in any theoretical calculation, there are always hidden assumptions! Hopefully I can give you a feel for some of them.

I. INTRODUCTION

According to the BaBar Physics Book[1], the purpose of lattice QCD calculations are "to solve QCD directly by a numerical simulation."

Wasn't QCD solved years ago?

Well, not really. For many years people have been able to calculate many non-perturbative quantities (hadron masses, decay constants) at the 10-20 per cent level, as long as the quark masses are not too small. That is not really accurate enough to be useful for Standard Model tests. We think we understand confinement and probably chiral symmetry breaking in the strong coupling limit of QCD, but there isn't a really convincing story about either of these in the continuum limit (like a graduate student would use in an oral exam). Lattice techniques got us the 10-20 per cent numbers, and will probably do a lot better in the next few years. There still may not be an answer for the graduate student.

The theory[2] behind the BaBar mission statement goes as follows: One begins with the generating functional for Green's functions for QCD, regulated with a UV cutoff, with a set of bare couplings, and the bare quark masses

$$Z(J) = \int [dA_\mu][d\psi][d\overline{\psi}] \exp(-S(\bar{\psi}, \psi, A)). \tag{1}$$

We integrate out the fermions, leaving the gauge action $S_G(A)$ and the fermion determinant,

$$Z(J) = \int [dA_\mu] \det(\not{D} + m)^{N_f} \exp(-S_G(A)). \tag{2}$$

A correlator is measured from (for example)

$$C_\Gamma(x, y) = \frac{1}{Z} \int [dA_\mu][d\psi][d\bar{\psi}] \exp(-S(\bar{\psi}, \psi, A))\bar{\psi}_x \Gamma \psi_x \bar{\psi}_y \Gamma \psi_y \tag{3}$$

which is equal to

$$C_\Gamma(x, y) = \int d^4q \sum_h \frac{|\langle 0|\bar{\psi}\Gamma\psi|h\rangle|^2}{q^2 + m_h^2} e^{iq(x-y)}, \tag{4}$$

and masses and matrix elements can be extracted from averages of $C_\Gamma(x, y)$: for example,

$$\int d^3\vec{x} C(x_0, \vec{x}; 0, 0) = \sum_h \exp(-m_h x_0) \frac{|\langle 0|\bar{\psi}\Gamma\psi|h\rangle|^2}{2m_h}. \tag{5}$$

The lattice comes in when one regulates the action with a UV cutoff which is a mesh of space-time points (with some lattice spacing a), replaces the continuum action with a lattice action (which is a function of bare parameters defined at the cutoff scale), and replaces the fields by some bare lattice fields. The bare action and fields are defined so that any desired symmetries survive discretization. Monte Carlo comes in when one replaces $Z(J)$ by an ensemble of "snapshots" of the gauge fields, where the probability of finding a particular snapshot is proportional to $\det(\not{D} + m)^{N_f} \exp(-S_G(A))$, and the correlator $C_\Gamma(x, y)$ is approximated by an ensemble average

$$\frac{1}{N} \sum_{j=1}^{N} C_\Gamma(x, y, \{A\}_j) + O(\frac{1}{\sqrt{N}}). \tag{6}$$

The ensemble of snapshots is generated on a computer. All lattice simulations are done at unphysical values of renormalized constants with a nonzero cutoff, in finite volume. Neglecting quark masses for the moment, a lattice calculation of a mass ratio will be a ratio of quantities measured in units of the cutoff, and will be equal to a cutoff-independent value plus a sum of cutoff effects

$$\frac{m_1(a)a}{m_2(a)a} = \frac{m_1}{m_2} + O(a) + O(a^2) + \dots . \tag{7}$$

Lattice people talk about "controlled systematic errors" when they think that they can take the volume to infinity, the lattice spacing to zero, and the quark mass to a physical value (that is, the bare quark mass from cutoff scale a is tuned so that some ratio of hadron masses takes its experimental value). The actual route from action to answer soon becomes horribly technical. Matrix elements of operators with anomalous dimensions have their own complications.

So much for ideology, on to reality. All theoretical calculations have hidden assumptions, and the lattice is no exception!

Lattice calculations begin by picking a lattice discretization of one's desired theory. In principle, constructing lattice actions is no different from the usual particle physics game that you all play: An author recognizes some interesting IR physics and invents a UV completion for his/her theory to get it. The interesting IR physics from the lattice was confinement: A lattice regulated gauge theory automatically confines if $g(a) \gg 1$. Now the UV cutoff is taken away (by taking $g(a)$ to zero). Maybe the desired IR physics will remain. Unfortunately, in addition to the desired IR physics, there will be a set of cutoff dependent corrections ($O(p^2a^2)$ for physics at scale p?). These effects are new physics. If they are not seen in experiment, one must argue that the energy scale for the UV completion is higher than some cutoff. The only difference between an ordinary particle theorist and a lattice theorist is that the ordinary theorist might believe that the UV completion corresponds to something real. For us, the lattice spacing is unphysical, and we know it. Nowadays we invent lattice actions which are designed to hide the cutoff effects, in order to do our simulations at larger values of the cutoff. This is called an "improvement program."

Unfortunately, this game has two big problems. First, cutoffs usually don't respect symmetries unless they are carefully designed. One will typically have to do some kind of fine-tuning of bare parameters to achieve symmetries in the IR. The lattice is particularly unfriendly to chiral symmetries, which is not good news for simulations with small quark mass.

The second problem is that numerical simulation is a big part of a lattice calculation, and computer resources are always finite. People naturally neglect things that they don't think are important. It may be hard to correct for this, later. Prime candidates are the extrapolation in quark masses, the use of the correct number of flavors, or the volume. Dynamical fermions are difficult because computers can't handle Grassman variables, so they are integrated out at the start. The fermion determinant is complicated and non-local. For years, lattice calculations have used the "quenched approximation," in which the number of dynamical flavors is set to zero: $\det(\not{D} + m)^{N_f} \to 1$. That's quite an approximation!

II. THE END OF QUENCHING

Lattice phenomenology dates from 1974, with Wilson's discovery of confinement[3] and (a bit later) of chiral symmetry breaking[4, 5] for lattice-regulated gauge theories. The first[6] Monte Carlo simulations were done in 1979, and the first calculations of hadron spectroscopy[7] date from 1981. A standard set of post-dictions (the proton mass) and predictions (B_K, f_B, f_D, ...) have been part of the lattice menu ever since. It is a subject characterized by gradual progress, punctuated by little revolutions. "QCD has been solved" – several times. Fig. 1 shows spectroscopy from 1994 [8]. You could not ask for anything better.

Of course, standards always go up, and QCD has been un-solved several times, too.

To view the past, we again visit the BaBar Physics Book (fall 1998), which has an appendix about lattice predictions. At that time, f_D and f_B had about a 20 MeV statistical uncertainty, B_B was a 5-10 % number, and $B_K = 0.61(6)$. The book did not quote a big systematic uncertainty: these calculations were done in quenched approximation. That apparently matters a lot for some quantities. Back then $f_{D_s} \simeq 220(30)$ MeV. A recent calculation[9] with 2+1 flavors gives $f_{D_s} = 263(+5, -9)(24)$ MeV (the two error bars are statistics and estimates of matching to the continuum), which is certainly not the same number. This illustrates the size of a "quenching systematic."

The quenched approximation has many of the ingredients of successful hadron phenomenology. Quarks are confined (with a linear confining potential if they are heavy). Chiral symmetry is spontaneously broken. In it, all states are (at first glance) infinitely narrow, because $q\bar{q}$ pairs cannot pop out of the vacuum. One might also try to "justify" the quenched approximation by an appeal to the quark model: in the quenched approximation, all mesons are $q\bar{q}$ pairs, and all baryons are qqq states. This also appears to be rather similar to the large-N_c limit of QCD.
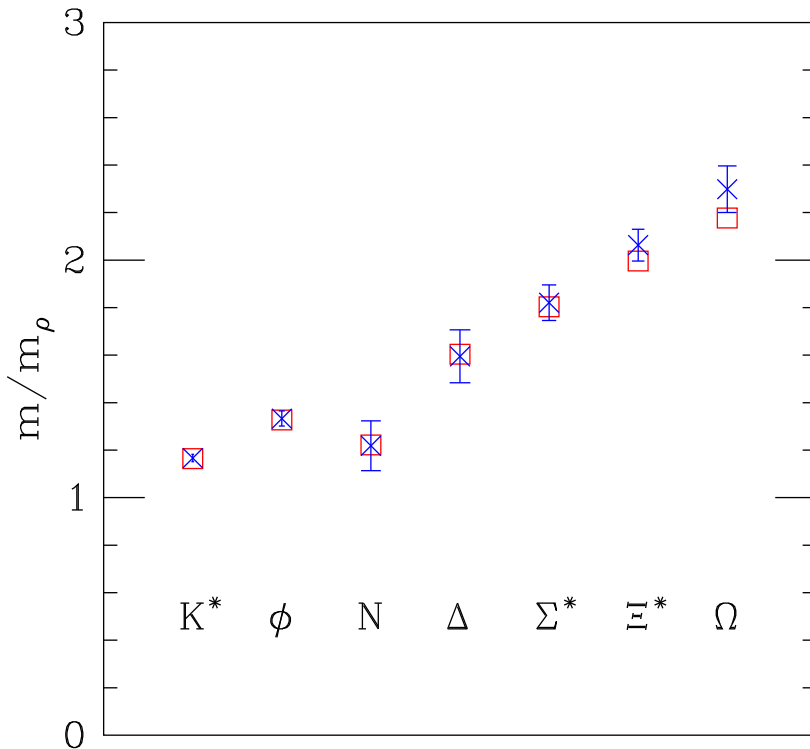
FIG. 1: Comparison of quenched results from Ref. [8] with experiment.

The best way to see what is going on in the quenched approximation is to consider the low energy limit of QCD. Do not think about quarks and gluons, but in terms of an effective field theory of QCD, described by a chiral Lagrangian in which the would-be Goldstone bosons are fundamental fields. These Lagrangians have a set of bare parameters (quark masses, f_π, the quark condensate Σ, ...). As far as the chiral Lagrangian is concerned, these are fundamental parameters. As far as QCD is concerned, one could compute these parameters from first principles (for example, $f_\pi m_\pi = \langle 0|\bar{\psi}\gamma_0\gamma_5\psi|\pi\rangle$), this would fix the parameters of the chiral Lagrangian, and then one could throw away the lattice and compute low energy physics using the chiral Lagrangian. Quenched QCD and QCD with nonzero flavor numbers are different theories and their low energy parameters will be different. But there is more. In full QCD the eta prime is heavy and can be decoupled from the interactions of the ordinary Goldstone bosons. In quenched QCD the eta prime is not really a particle. The would-be eta prime gives rise to "hairpin insertions" which pollute essentially all predictions.

Let's consider the eta prime channel in full QCD and quenched QCD. In ordinary QCD, the eta prime propagator includes a series of terms in which the flavor singlet $q\bar{q}$ pair annihilates into some quarkless state, then reappears, over and over. This is shown in Fig. 2. The eta prime propagator is

$$\eta'(q) = C(q) - H_0(q) + H_1(q) + \ldots \tag{8}$$

where $C(q) = 1/d$, $d = q^2 + m_\pi^2$, is the "connected" meson propagator, the same as for any other Goldstone boson. H_n is the nth order hairpin (with n internal fermion loops). Assuming that each vertex is a constant V allows us to sum the geometric series

$$\eta'(q) = \frac{1}{q^2 + m_\pi^2 + V} \tag{9}$$

and generate a massive eta prime.

However, the quenched limit is different – there are no loops. In that case (of course)

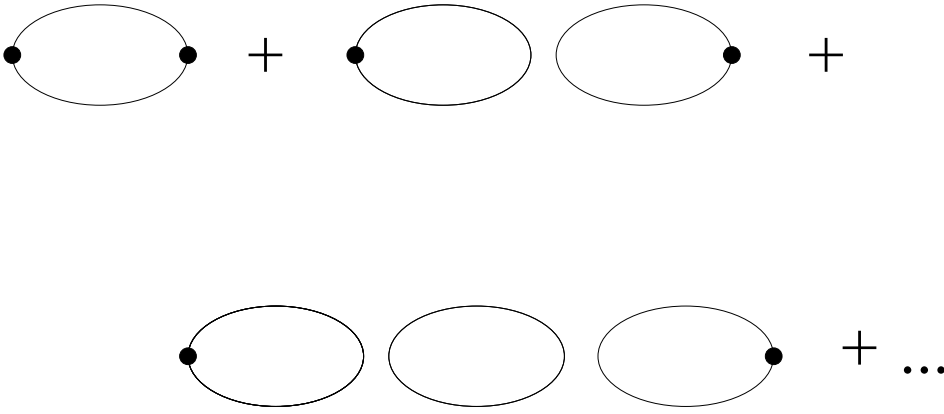$$\eta'(q) = \frac{1}{d} - \frac{1}{d}V\frac{1}{d}. \tag{10}$$

FIG. 2: The eta-prime propagator in terms of a set of annihilation graphs summing into a geometric series to shift the eta-prime mass away from the mass of the flavor non-singlet pseudoscalar mesons. In the quenched approximation, only the first two terms in the series survive as the "direct" and "hairpin" graphs.

In the eta prime channel there is an ordinary (but flavor singlet) Goldstone boson and a new contribution–a double-pole ghost (negative norm) state. In the $N_c = \infty$ limit, the double pole decouples, but finite N_c quenched QCD remains different from finite N_c full QCD. The limits of large N_c and quenching don't commute.

Where the eta-prime comes in is in the calculation of corrections to tree-level relations[10, 11]. These are typically dominated by processes with internal Goldstone boson loops, contributing terms like

$$\int d^4 k G(k, m) \simeq (\frac{m}{4\pi})^2 \log(\frac{m^2}{\Lambda^2}) \tag{11}$$

(plus cutoff effects). The eta-prime hairpin can appear in these loops, replacing $G(k, m)$ by $-G(k, m)VG(k, m)$ and altering the chiral logarithm. Thus, in a typical observable, with a small mass expansion

$$Q(m_{PS}) = A(1 + B\frac{m_{PS}^2}{f_{PS}^2} \log m_{PS}^2) + \ldots \tag{12}$$

quenched and $N_f = 3$ QCD can have different coefficients (different B's in Eq. 12), seemingly randomly different. (Quenched f_π has no chiral logarithm while it does in full QCD, the coefficient of O_+, the operator measured for B_K, is identical in quenched and full QCD, etc.) Even worse, one can find a different functional form. For example, the relation between pseudoscalar mass and quark mass in full QCD is

$$m_{PS}^2 = Am_q(1 + \frac{m_{PS}^2}{8\pi^2 f^2} \log(m^2/\Lambda^2)] + \ldots. \tag{13}$$

In quenched QCD, the analogous relation is

$$(m_{PS})^2/(m_q) = A[1 - \delta(\ln(m^2/\Lambda^2) + 1)] + \ldots \tag{14}$$

where $\delta = V/(8\pi^2 N_c f_\pi^2)$ is expected to be about 0.2 using the physical η' mass. This means that m_{PS}^2/m_q actually diverges in the chiral limit!

While there are some lattice observations of Eq. 14 behavior (with noisy fits to δ), what has really happened is a crisis in confidence for phenomenology. People want to extrapolate their simulations (run at unphysically heavy quark masses) to the chiral limit. The best way to do that is to use an effective chiral Lagrangian to predict the quark mass dependence. But if the chiral Lagrangian which describes quenched QCD is different from the one which describes $N_f = 3$ QCD, what are you supposed to do? (And for that matter, if quenched QCD and $N_f = 3$ QCD are really different theories, how can you be sure that matching one or a few parameters means that other quantities will match?)

And since people think that they can do simulations with dynamical quarks, why bother with the quenched approximation?

We are in the middle of an exciting and peculiar time in lattice QCD, with two essentially uncoupled developments. One of them is much more mature from the point of simulations, and it is the one which has gotten most of the publicity lately: these are simulations with three flavors of light dynamical fermions. The other development is the discovery of lattice fermion actions which have exact chiral symmetry at nonzero lattice spacing.

III. LATTICE CHIRAL SYMMETRY IN A NUTSHELL

A naive discretization of the Dirac operator $\bar{\psi}\gamma_\mu p_\mu \psi \to \bar{\psi}\gamma_\mu \frac{1}{a}\sin(p_\mu a)\psi$ "doubles" the spectrum; all the modes with at least one $p_\mu \simeq \pi/a$ are as light as the one near $p = 0$, and the chiral charges of all the doublers plus the $p = 0$ mode cancel. The Nielsen-Ninomiya[15] theorem encodes a dilemma: no lattice fermion action can be quadratic, well-behaved, have a conserved local axial charge Q which is quantized, without having an equal number of left-handed and right-handed fermions for each eigenvalue of Q. Thus the "classic" division of lattice fermions into Wilson-like, which have no doublers but explicitly break chiral symmetry with higher dimensional operators, or staggered fermions, which double but preserve a relic of chiral symmetry. An exact transformation and decimation to one-component fermions living on the sites of a hypercube shrinks the multiplicity-16 naive fermion into a multiplicity-4 staggered fermion, and leaves a $U(1) \otimes U(1)$ chiral symmetry.

However, if something is really forbidden, they don't pass laws against it, and smart people invented a number of ways to evade the Nielsen-Ninomiya theorem. Back in 1982, Ginsparg and Wilson[16] used renormalization group ideas to propose a modification of chiral symmetry; they could not provide an explicit example of an action and the idea was forgotten. In the early '90's Kaplan[17] and later Shamir[18] studied QCD in five-dimensional worlds. The sound bite is very familiar: we (and the chiral fermion) live on a brane or a boundary in a higher dimensional world. A transfer matrix version of this idea, to give a chiral four-dimensional fermion, was developed at the same time by Narayanan and Neuberger[19]. (Quenched) simulations with domain wall fermions began in 1996. The bulk of the community not doing these things woke up in 1997 when the Ginsparg-Wilson was rediscovered, and Luscher[20] gave us a modified rule for a chiral transformation involving the Dirac operator D itself

$$\delta\psi = \epsilon\gamma_5(1 + aD)\psi; \qquad \delta\bar{\psi} = \epsilon\bar{\psi}\gamma_5 \tag{15}$$

which encodes the Ginsparg-Wilson relation,

$$\{\gamma_5, D\} = aD\gamma_5 D. \tag{16}$$

This is a magic formula, which guarantees that all chiral Ward identities are satisfied on the lattice at nonzero lattice spacing, up to contact terms. The index theorem is exact for the overlap: topology can be computed by counting fermionic chiral zero modes. The overlap of Neuberger and Narayanan is an exact[21] realization of the Ginsparg-Wilson relation, using any nonchiral Dirac operator d, it is

$$D_{ov} = R_0(1 + \frac{d - R_0}{\sqrt{|d - R_0|^2}}) \tag{17}$$

With it, quenched simulations with the overlap began a year or two later.

IV. STAGGERED FERMION PHENOMENOLOGY

With essentially zero overlap with the last section, since 1987 members of the MILC collaboration have been doing simulations of QCD with two and three flavors of ever lighter dynamical staggered fermions. By 2001 they were up to lattice sizes of $L = 2\ fm$, and down to a strange quark at its physical value, and non-strange quarks of about 20 MeV. A major problem with staggered quarks is that interactions mix and split hadrons made of the four "tastes" of a single staggered flavor, producing a pattern reminiscent of the effect of crystal fields on atomic spectral lines (See Fig. 3 for an example.) The MILCmen[12] redesigned the gauge connection of the staggered fermion to reduce this to a small value, so that it made sense to talk about pions and kaons as separated collections of states.

Their next crucial breakthrough was in the data analysis: The key to doing this was provided by the Sharpe and Lee[13] analysis of taste mixing, whose construction of a low energy chiral effective theory including explicit taste-breaking interactions predicted the degeneracies shown in Fig. 3. Their work was generalized by Aubin and Bernard[14]. One writes down a low energy effective field theory for the Goldstones (Σ is the usual exponential of the particle fields, traces over the 16 taste-product $q\bar{q}$ bilinears of each staggered flavor) and the Lagrangian is

$$\mathcal{L} = \frac{f^2}{8}\text{Tr}(\partial_\mu\Sigma\partial_\mu\Sigma^\dagger) - \frac{1}{4}\mu f^2\text{Tr}(\mathcal{M}\Sigma + \mathcal{M}\Sigma^\dagger) \tag{18}$$

$$+\frac{2m_0^2}{3}(U_I + D_I + S_I + \cdots)^2 + a^2\mathcal{V}, \tag{19}$$

where the m_0^2 term weighs the analog of the flavor singlet η'. (The "I" subscripts display that this involves the taste singlet term for each flavor.) The $a^2\mathcal{V}$ term is the taste-breaking interaction, a sum of terms quadratic in Σ with
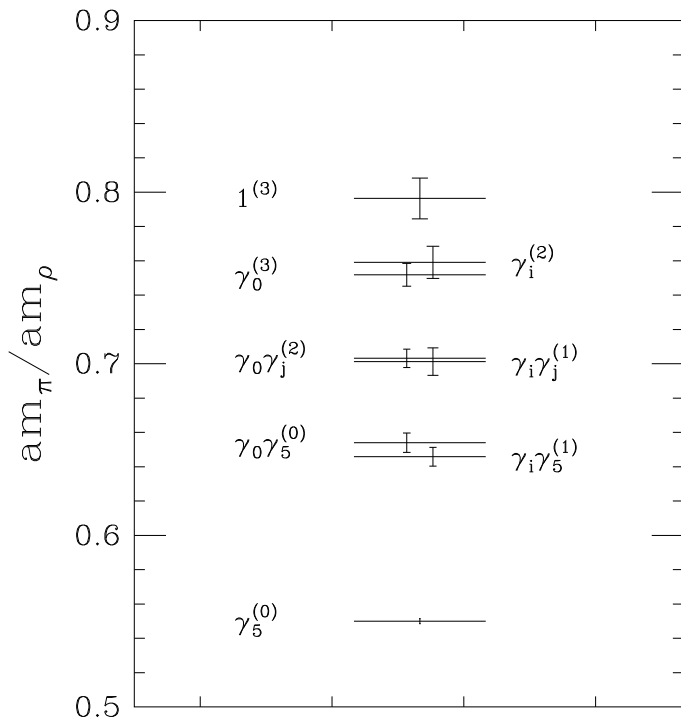
FIG. 3: An example of flavor or taste symmetry breaking in an improved staggered action. The different γ's are a code for the various pseudoscalar states. Data are from Ref. [12]. For an explanation of the splitting, see Ref. [13].

various taste projectors, parameterized by six coefficients (only one is big). Now one computes "any" desired quantity with this Lagrangian, typically to one loop, as a function of quark masses and all other coefficients. Parameters of Nature are determined when mass-dependent Monte Carlo data is fit to this functional form. For example, a one-loop fit to $m_{PS}/(m_1 + m_2)$ for the true would-be Goldstone boson made of quarks of mass m_1 and m_2 would involve μ, f, two Gasser-Leutwyler parameters, three otherwise unconstrained lattice parameters, and involves chiral logarithms whose arguments are all the observed pseudoscalar masses. Fits to f_π or f_K are similar.

Last year they were joined by other collaborations who used their configurations for backgrounds for other QCD phenomenology. In Ref. [22] they presented results for a variety of post-dictions, which certainly set a new standard for claimed precision: Fig. 4 shows their results.

Since then, they and their collaborators have gone on to do

- The strong coupling constant at the Z-mass: see Fig. 5

- Form factors for semileptonic B and D meson decay (See Ref. [23]).

- Marciano[24] has proposed using a lattice calculation of f_K/f_π to fix V_{us}. The present MILC data give $V_{us} = 0.2236(30)$; the error is compatible with other determinations of V_{us} and can be shrunk by better lattice simulations

- Blum [25] is using MILC data to compute the hadronic contribution to muon $(g - 2)$ from first principles

- MILC[26] has determined the strange and nonstrange quark masses from a fit to spectroscopy: $m_s(\overline{MS}, \mu = 2$ GeV$) = 76$ MeV, the nonstrange average mass 2.8(4) MeV. The up quark is not massless, by many standard deviations.

and much more.

There is one problem with the MILC results, however. Recall that a single staggered flavor corresponds to four continuum "tastes." To get the correct flavor weighting of the determinant, MILC takes the quarter root of the staggered determinant so that $\det(D_{stagg})^{1/4}$ approximates $\det(D_{1-flavor})$. If we had a theory of 4 degenerate fermions, each with its Dirac operator D_1, then one could define an operator $D_4 = (D_1)^4$ and then $\det(D_4)^{1/4} =$
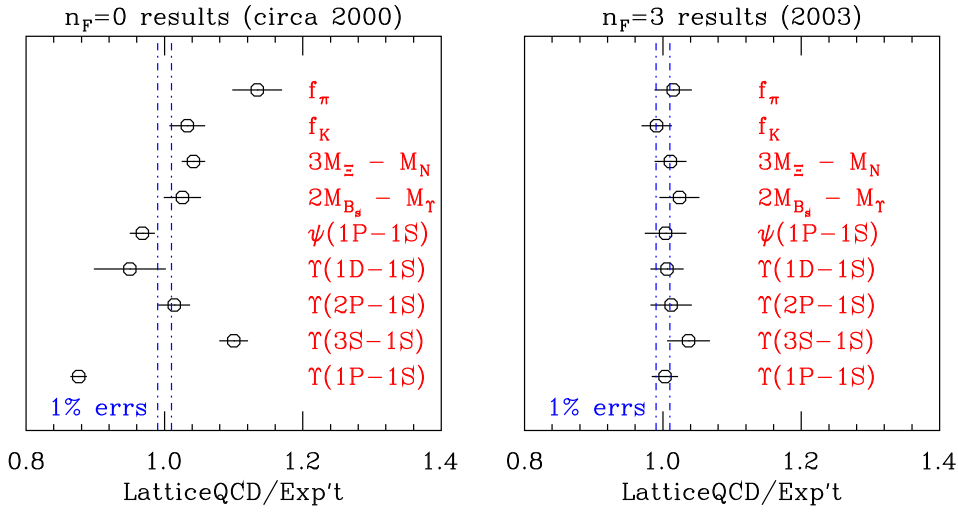
FIG. 4: Comparison of quenched results with results from simulations with 2+1 flavors of staggered fermions, from Ref. [22]
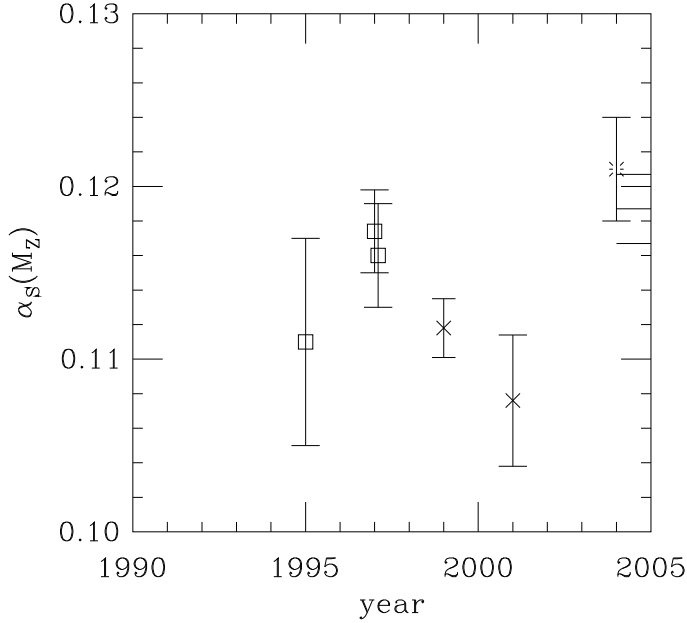

FIG. 5: Lattice calculations of $\alpha_{\overline{MS}}(M_Z)$ vs. year of publication. The burst is the recent 2+1 flavor result of Ref. [22]. Squares are earlier staggered simulations, mostly with two flavors of dynamical simulations, and the crosses are $N_f = 2$ simulations with clover or Wilson fermions. The horizontal lines along the right edge show the one-sigma PDG average.

$\det D_1$. But gluons introduce taste-breaking terms among the four tastes, and it is not clear if there is an analog of D_1 for staggered fermions, which is theoretically well behaved. If it exists, it would be undoubled and chiral. It would, therefore, collide with the Nielsen-Ninomaya theorem unless its chiral properties are unusual.

Lattice simulations treat valence quarks and sea quarks differently. Basically, valence quarks (the ones attached to the external sources) do not have any quantum numbers (other than their mass and spin). One computes classes of Feynman diagrams on the lattice, then re-weights them with global symmetry indices and bundles them together. (For example, the same propagators are used for the quark and the antiquark in a mass-degenerate meson). In staggered fermions, one uses a single flavor of staggered fermions, with its four tastes, and computes correlation functions in

which the sources project (nearly) onto the same initial and final taste. The quark could hop temporarily into a different taste state as it propagates across the lattice (this would happen by emitting and absorbing hard gluons), but this is just cutoff scale physics which contributes $O(a^2 g^2)$ scale violations. It is the sea quarks which are the problem, and it is a peculiar exchange of limits problem: if taste symmetry were exactly restored below some lattice spacing, then the spectrum would be exactly degenerate, and the fractional determinant would correctly count the eigenvalues of a single flavor. But that is not what happens, taste symmetry is only restored in the continuum limit.

The whole business is very puzzling and unresolved. Some people who worry about this believe that there is no local action whose determinant is equal to $\det(D_{stagg})^{1/4}$, so that the theory which is being simulated is not a legitimate quantum field theory. Being nonlocal is bad: there is no possibility of a renormalization group, because short distance physics cannot be integrated out. This means universality is lost.

Bunk, Della Morte, Jansen and Knechtli [27] did a direct study of whether $D_{stagg}^{1/4}$ (actually a form of $(D_{stagg}^\dagger D_{stagg})^{1/2}$) was local. It was not. However, saying that two matrices have identical determinants is not the same as saying that the matrices are identical. All one needs is to find one local D, the fact others are nonlocal is not important.

A number of people[28, 29, 30] have done comparisons of staggered fermions with overlap fermions, with various choices for discretizations of the gauge-fermion connections. They compare the spectrum of Dirac eigenmodes in various circumstances. Staggered fermions do not have zero modes in the presence of instantons, but people find that they can tune these actions to make the modes more and more small and degenerate. To me, this is more a statement about the properties of the valence quarks than an exact result about determinants.

Dürr and Hoelbling[31] have done simulations of the Schwinger model with fractional powers of staggered quarks and with overlap quarks. They ascribe the differences they see to cutoff dependence of the staggered quarks, but do not see a smoking gun of anything going obviously wrong.

Finally, there has been a spate of activity with free field theory. Maresca and Peardon [32] have constructed local actions for free fermions whose dispersion relation is equal to that for staggered fermions and whose determinant is equal to $\det(D_{stagg})^{1/4}$. They must impose a Ginsparg-Wilson type of chiral rotation on their construction to get locality for this transformation. Adams[33] has proposed a Wilson-type action with the same determinant. Shamir[34] has shown that under RG transformations the free staggered action blocks into an action with a quadrupled spectrum (in the limit of infinitely many blocking steps). If one could construct such an action[35] and use it in simulations, all would be well, but that is not what is done in practice: the fractional root is taken first. It is unknown if any of these constructions can be extended to the interacting theory.

MILC's fits to the staggered chiral Lagrangian include the effective number of sea quarks as a free parameter. They have done fits freeing it, and find[26] (for various observables) 1.2 to 1.4 "fit flavors" (with an uncertainty of about 0.2) where 1 is desired.

None of these studies constitutes a demonstration for or against the $\det^{1/4}$ trick. For the present, MILC data must dominate any phenomenological analysis which needs a hadronic matrix element to constrain a Standard Model parameter. It is clearly better than a quenched calculation, and their quark masses are the smallest ones being simulated. No other formulation of fermions allows simulations at such small quark masses nor large volumes: it is hard to get below $m_{PS}/m_V = 0.6$.

V. CONCLUSIONS

Lattice people are hard at work. The MILC program is a major part of American lattice QCD. Many groups are using their configurations to do phenomenology including the effects of dynamical fermions. MILC is doing upgrades – dropping the quark mass and the lattice spacing, raising the volume. These are the best lattice numbers for phenomenologists to use to date. But the problem remains: is the $\det^{1/4}$ trick a controlled approximation, or not?

Simulations with dynamical chiral fermions are just beginning. The RBC collaboration[36] is doing production runs with big lattices with $N_f = 2$ flavors of domain wall fermions. Dynamical overlap simulations are theoretically clean and beautiful, but remote. Only a few visionary (?) people are playing with them[37]. For you at this conference, the most interesting message might be that such methods are beginning to appear; one might be able to cleanly address interesting questions about chiral fermions with them.

Presumably QCD will be solved a few more times before we retire.

Acknowledgments

This work was supported by the US Department of Energy.

[1] P. F. Harrison and H. R. . Quinn [BABAR Collaboration], SLAC-R-0504 *Papers from Workshop on Physics at an Asymmetric B Factory (BaBar Collaboration Meeting), Rome, Italy, 11-14 Nov 1996, Princeton, NJ, 17-20 Mar 1997, Orsay, France, 16-19 Jun 1997 and Pasadena, CA, 22-24 Sep 1997*

[2] For a recent review of lattice QCD, see T. DeGrand, Int. J. Mod. Phys. A **19**, 1337 (2004) [arXiv:hep-ph/0312241].

[3] K. G. Wilson, Phys. Rev. D **10**, 2445 (1974).

[4] J. Greensite and J. Primack, Nucl. Phys. B **180**, 170 (1981).

[5] H. Kluberg-Stern, A. Morel, O. Napoly and B. Petersson, Nucl. Phys. B **190**, 504 (1981).

[6] M. Creutz, L. Jacobs and C. Rebbi, Phys. Rev. Lett. **42**, 1390 (1979).

[7] H. Hamber and G. Parisi, Phys. Rev. Lett. **47**, 1792 (1981).

[8] F. Butler, H. Chen, J. Sexton, A. Vaccarino and D. Weingarten, *Nucl. Phys.* **B 430** 179 (1994) [arXiv:hep-lat/9405003].

[9] J. N. Simone *et al.* [MILC Collaboration], arXiv:hep-lat/0410030.

[10] C. W. Bernard and M. F. Golterman, *Phys. Rev.* **D 46** 853 (1992) [hep-lat/9204007]; *Phys. Rev.* **D 49**, 486 (1994) [arXiv:hep-lat/9306005].

[11] S. R. Sharpe, *Phys. Rev.* **D 46** 3146 (1992) [arXiv:hep-lat/9205020].

[12] K. Orginos, D. Toussaint and R. L. Sugar [MILC Collaboration], Phys. Rev. D **60**, 054503 (1999) [arXiv:hep-lat/9903032].

[13] W. J. Lee and S. R. Sharpe, *Phys. Rev.* **D 60** 114503 (1999) [arXiv:hep-lat/9905023].

[14] C. Aubin and C. Bernard, *Phys. Rev.* **68** 074011 (2003) [arXiv:hep-lat/0306026]; *Phys. Rev.* **D 68** 034014 (2003) [arXiv:hep-lat/0304014]. C. Aubin *et al.* [MILC Collaboration], "Pion and kaon physics with improved staggered quarks," arXiv:hep-lat/0309088.

[15] H. B. Nielsen and M. Ninomiya, Nucl. Phys. B **185**, 20 (1981) [Erratum-ibid. B **195**, 541 (1982)], Nucl. Phys. B **193**, 173 (1981).

[16] P. H. Ginsparg and K. G. Wilson, Phys. Rev. D **25**, 2649 (1982).

[17] D. B. Kaplan, Phys. Lett. B **288**, 342 (1992) [arXiv:hep-lat/9206013].

[18] Y. Shamir, Nucl. Phys. B **406**, 90 (1993) [arXiv:hep-lat/9303005].

[19] R. Narayanan and H. Neuberger, Nucl. Phys. B **443**, 305 (1995) [arXiv:hep-th/9411108]. Phys. Rev. Lett. **71**, 3251 (1993) [arXiv:hep-lat/9308011]. Nucl. Phys. B **412**, 574 (1994) [arXiv:hep-lat/9307006]. Phys. Lett. B **302**, 62 (1993) [arXiv:hep-lat/9212019].

[20] M. Luscher, Phys. Lett. B **428**, 342 (1998) [arXiv:hep-lat/9802011].

[21] H. Neuberger, Phys. Lett. B **417**, 141 (1998) [arXiv:hep-lat/9707022].

[22] C. T. Davies *et al.* [HPQCD Collaboration], "High-precision lattice QCD confronts experiment," arXiv:hep-lat/0304004.

[23] A. Gray, C. Davies, E. Gulez, G. P. Lepage, J. Shigemitsu and M. Wingate, arXiv:hep-lat/0409040.

[24] W. J. Marciano, arXiv:hep-ph/0402299.

[25] T. Blum, arXiv:hep-lat/0411002.

[26] C. Aubin *et al.* [MILC Collaboration], arXiv:hep-lat/0407028.

[27] B. Bunk, M. Della Morte, K. Jansen and F. Knechtli, Nucl. Phys. B **697**, 343 (2004) [arXiv:hep-lat/0403022].

[28] E. Follana, A. Hart and C. T. H. Davies [HPQCD Collaboration], Phys. Rev. Lett. **93**, 241601 (2004) [arXiv:hep-lat/0406010].

[29] E. Follana, C. Davies, A. Hart, P. Lepage, Q. Mason and H. Trottier [HPQCD Collaboration], Nucl. Phys. Proc. Suppl. **129**, 384 (2004) [arXiv:hep-lat/0406021].

[30] K. Y. Wong and R. M. Woloshyn, arXiv:hep-lat/0412001.

[31] S. Durr and C. Hoelbling, Phys. Rev. D **69**, 034503 (2004) [arXiv:hep-lat/0311002]; arXiv:hep-lat/0411022.

[32] F. Maresca and M. Peardon, arXiv:hep-lat/0411029.

[33] D. H. Adams, arXiv:hep-lat/0411030.

[34] Y. Shamir, arXiv:hep-lat/0412014.

[35] W. Bietenholz, R. Brower, S. Chandrasekharan and U. J. Wiese, Nucl. Phys. B **495**, 285 (1997) [arXiv:hep-lat/9612007].

[36] Y. Aoki *et al.*, arXiv:hep-lat/0411006.

[37] Z. Fodor, S. D. Katz and K. K. Szabo, JHEP **0408**, 003 (2004) [arXiv:hep-lat/0311010]. N. Cundy, S. Krieg, A. Frommer, T. Lippert and K. Schilling, arXiv:hep-lat/0409029. T. DeGrand and S. Schaefer, arXiv:hep-lat/0412005.